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us to make further divisions so as finally to reach continuous and consistent areal units. I formed my division into life districts according to the primary conditions of life, and I never claimed that all the localities on the earth showing the same primary conditions of life should be continuous; I only claimed that the smallest areal units of zoögeographical division should be continuous. Different conditions of life have existed since the beginning of the geological history of the earth; the secondary divisions into regions of the marine life districts, which were formerly continuous in a greater or less degree, are made according to the topographical continuity, which was interrupted by the introduction of climatic differences in much later times. The assigned districts of life are old, and during a long time they were the only zoögeographical divisions of the seas. The different regions of the life districts are of a comparatively recent date, and their existence did not begin until a differentiation of climate took place.

Prof. Gill further suggests that the life districts themselves are of unequal value, and they should be segregated into two primary categories, marine and inland. I agree perfectly with this view, as the same view is maintained in my book, the title of which reads: '*Principles of marine zoögeography*,' thus leaving out of view the consideration of *inland* districts. Further, I expressly state (p. 18-20) that the diagnostic value of my five life districts differs, for if we were to establish a perfectly philosophical division we should have to introduce other districts, but only the five named are of *practical* value. The fact that the marine life districts are unequal as regards the number of subdivisions I cannot consider as an objection to their correctness. Indeed, in this respect they *are* unequal, but if they are unequal in nature why should we try to correct nature in proposing a scheme on paper in which the divisions would appear more equal than they really are?

I am glad that Prof. Gill by his remarks has given me an occasion to state again in a concise form my reasons for neglecting the inductive or statistical method in zoögeography. I think that practical results favor my method, es-

pecially since there is a remarkable parallelism in both divisions, Prof. Gill's and mine. This fact suggests that an agreement of both is at least possible, and then, perhaps, some of the scientific terms of Prof. Gill would have the priority and should be used, as most of the terms used by me are certainly in that particular sense of more recent date.

ARNOLD E. ORTMANN.

PRINCETON COLLEGE, May, 1896.

'THE CHILD AND CHILDHOOD IN FOLK-THOUGHT.'

TO THE EDITOR OF SCIENCE: In the issue of March 27th Dr. Brinton has dwelled on the literary merits of Dr. A. F. Chamberlain's book '*The Child and Childhood in Folk-Thought*.' As, aside from its literary aspirations, the book is intended as a contribution to Anthropological Science, I may be permitted to add a few words from this point of view.

Dr. Brinton has well said that the book represents a vast amount of compilatory work. The author deserves our thanks for having delved in numerous odd books in which we should hardly expect to find information on the subject of childhood, and for having extricated a considerable number of references from ethnological literature. He has thus largely increased the available material on studies of childhood. These references he has conveniently arranged in a bibliographical index.

While this preparatory work is very meritorious, particularly in so far as it refers to uncommon books, the attempt at a scientific arrangement of the material thus obtained does not appear successful. If scientific description was the author's aim it was incumbent upon him to arrange his material from certain points of view in a systematic way. If he desired by inductive methods to investigate certain phenomena it was his duty to array his facts for the purpose of finding the elements common to all of them. His book fills neither the one nor the other requirement.

A characteristic instance of lack of organic connection is the seventh chapter, '*Affection for Children*.' The subject-matter treated is as follows: Parental love, the dead child, motherhood and infanticide, the dead mother, fatherly

affection, kissing, tears, cradles, father and child.

The sixth chapter, 'Primitive Child-Study' or 'The Child in the Primitive Laboratory,' embraces the following headings: Licking into shape, massage, face games, primitive weighing, primitive measurements, measurements of limbs and body, tests of efficiency, sleep, heroic treatment.

I believe these two statements show that the points of view, according to which the author has coördinated his material, are based entirely on considerations foreign to it. This is particularly clear in the sixth chapter. The various customs collated there have hardly any psychological connection and can, therefore, not be held to elucidate in any way the mode of thought of primitive man. He neither thinks of studying children—as we are just beginning to do—nor does he subject them to tests. The customs recorded by the author are practiced for a variety of purposes, but, certainly, the fact that they resemble in a general way tests which we might apply does not give us a right to consider them as psychically connected.

Almost the only chapters in which we can find a connecting idea are the philological ones with which the book opens. In these the author makes a compilation of the uses to which the terms 'father' and 'mother' have been put by various people. But here another lack of the whole work becomes particularly glaring. The quotations are gleaned without any attempt at criticism, and much of the material that is offered is not a safe guide to follow, because the observations and investigations of the writers referred to were not sufficiently thorough.

The book is an illustration of the dangers with which the comparative method of anthropological investigation that has come into vogue during the last quarter of a century is beset.

The fundamental idea of this method, as outlined by Tylor and in the early writings of Bastian, is the basis of modern anthropology, and every anthropologist must acknowledge its soundness.

But with its growth have sprung up many collectors who believe that the mere accumulation of more or less similar phenomena will advance science. In every other science the

material on which induction is based is scanned and scrutinized in the most painstaking manner before it is admitted as evidence. It is absurd to believe that anthropology is entitled to disregard this rule, which is acknowledged as fundamental in all other inductive sciences. Furthermore, the object of anthropological research being to elucidate psychological laws on the one hand and to investigate the history of human culture on the other, we must consider it a primary requirement that only such phenomena are compared as are derived psychologically or historically from common causes. How this can be done has been shown by no one better than by Tylor. Only the common mistake of attributing any two phenomena that are somewhat alike to a common cause can explain the reasoning that led the author to amass and to place side by side entirely heterogeneous material.

I believe anthropologists, by silently accepting as a contribution to science a compilation like the present made on unscientific principles, will give countenance to the argument that has been brought so often against anthropology as a branch of science: namely, that it is lacking in a well defined scientific method and that, therefore, it is not equal in rank to other sciences.

FRANZ BOAS.

NEW YORK, May 1st, 1896.

THE DISCUSSION OF INSTINCT.

TO THE EDITOR OF SCIENCE: I have been much interested in the letters in your columns on the instinctive activities of young birds. Certain opinions which I hold—and others that the writers suppose that I hold—have been criticised. To explain my exact position, however, would occupy more space than I can reasonably ask you to afford me. May I be allowed, therefore, to content myself with stating that I have in preparation a work on *Habit and Instinct* which will, I hope, be published towards the close of this year. There my own observations will be described and reference will be made to the work of other observers, and there the provisional conclusions drawn from such observations will be discussed. I desire to make this statement, lest my silence should be regarded as discourteous in the coun-